



King's Research Portal

DOI:

[10.1017/S0007123419000449](https://doi.org/10.1017/S0007123419000449)

Document Version

Peer reviewed version

[Link to publication record in King's Research Portal](#)

Citation for published version (APA):

Giani, M., & Méon, P-G. (2019). Global Racist Contagion Following Donald Trump's Election. *BRITISH JOURNAL OF POLITICAL SCIENCE*, 0(0), 1 - 8. <https://doi.org/10.1017/S0007123419000449>

Citing this paper

Please note that where the full-text provided on King's Research Portal is the Author Accepted Manuscript or Post-Print version this may differ from the final Published version. If citing, it is advised that you check and use the publisher's definitive version for pagination, volume/issue, and date of publication details. And where the final published version is provided on the Research Portal, if citing you are again advised to check the publisher's website for any subsequent corrections.

General rights

Copyright and moral rights for the publications made accessible in the Research Portal are retained by the authors and/or other copyright owners and it is a condition of accessing publications that users recognize and abide by the legal requirements associated with these rights.

- Users may download and print one copy of any publication from the Research Portal for the purpose of private study or research.
- You may not further distribute the material or use it for any profit-making activity or commercial gain
- You may freely distribute the URL identifying the publication in the Research Portal

Take down policy

If you believe that this document breaches copyright please contact librarypure@kcl.ac.uk providing details, and we will remove access to the work immediately and investigate your claim.



King's Research Portal

Document Version
Peer reviewed version

[Link to publication record in King's Research Portal](#)

Citation for published version (APA):

Giani, M., & Méon, P-G. (Accepted/In press). Global Racist Contagion Following Donald Trump's Election. BRITISH JOURNAL OF POLITICAL SCIENCE.

Citing this paper

Please note that where the full-text provided on King's Research Portal is the Author Accepted Manuscript or Post-Print version this may differ from the final Published version. If citing, it is advised that you check and use the publisher's definitive version for pagination, volume/issue, and date of publication details. And where the final published version is provided on the Research Portal, if citing you are again advised to check the publisher's website for any subsequent corrections.

General rights

Copyright and moral rights for the publications made accessible in the Research Portal are retained by the authors and/or other copyright owners and it is a condition of accessing publications that users recognize and abide by the legal requirements associated with these rights.

- Users may download and print one copy of any publication from the Research Portal for the purpose of private study or research.
- You may not further distribute the material or use it for any profit-making activity or commercial gain
- You may freely distribute the URL identifying the publication in the Research Portal

Take down policy

If you believe that this document breaches copyright please contact librarypure@kcl.ac.uk providing details, and we will remove access to the work immediately and investigate your claim.

Global Racist Contagion Following Donald Trump's Election

September 23, 2019

Abstract

Exploiting the coincidence between the timing of U.S. presidential elections and the fieldwork period of the European Social Survey, we show that Donald Trump's win significantly increased self-reported racial bias in policy attitudes outside the U.S. We document that the opposite occurred following Barack Obama's first election in 2008, while no effect occurred when he or George W. Bush were reelected in 2012 and 2004. We show that the increase in self-reported racial bias is not driven by welfare-related immigration concerns, campaign effects, or bandwagon effects, suggesting a decrease in the social desirability of racial equality.

1 Introduction

The election of Donald Trump was followed by a spike in the number of hate crimes and online harassment targeting minorities (Hauslohner Abigail, 2016, Potok, 2017, Leving and Grisham, 2017, Müller and Schwarz, 2018). One explanation holds that this sequence of events reflected a shift in social norms: While pro-racial equality attitudinal trends in the U.S. had spread optimism about the future of race-relationship, Donald Trump’s win signaled that social norms had shifted towards a greater acceptance of racist attitudes (Bursztyn, Egorov and Fiorin, 2017; Crandall, Miller and White, 2018; Rushin and Sims, 2018).

While the available evidence pertains to the U.S., concerns that Donald Trump’s win legitimized racist attitudes abroad were voiced in media across the globe (see e.g. Shabi, 2016). *Al-Jazeera* even worried that “Trump’s electoral victory has been a wake call for all democratic nations to consider the solidification of the global right-wing and discriminatory politics in Europe and beyond” (Cherkaoui, 2016). Were those concerns founded? To answer that question, we test whether the election of Donald Trump increased racial bias in policy attitudes outside the U.S.

To identify the effect of Donald Trump’s election, we exploit the coincidence of the 2016 U.S. presidential election with the fieldwork period of 13 developed countries sampled by the European Social Survey (*ESS*). The *ESS* provides individual-level information about political attitudes, including attitudes on race-targeting policies. Most of all, the day of the interview can be considered as good as random with respect to the day of the election (Bar-Tal and Labin, 2001; Perrin and Smolek, 2009; Legewie, 2013). Using a quasi-experimental approach, we compare self-reported racial bias among respondents interviewed after the election (the treatment group) and those interviewed before the election (the control group).

We find evidence that the probability to report a racial bias increased by 2.3 percentage points within an interval of ± 15 days around the election of Donald Trump. The treatment effect is statistically significant and robust to several econometric specifications, including different sets of controls, time intervals, clustering, and covariate-balancing strategies. As the *ESS* has typically been run from Septem-

ber to January every even year since 2002, we can replicate the main analysis for previous U.S. elections. The main result is unlikely to be spurious: We find that self-reported racial bias significantly decreased when Barack Obama, arguably the near-perfect opposite of Donald Trump on race-related issues, won a first mandate. Conversely, elections that did not change the status quo, as when George W. Bush and Barack Obama received a second mandate in 2004 and 2012, had no significant effect on self-reported attitudes displaying racial bias.

We interpret our findings in light of the literature studying the effect of social norms on the report of sensitive attitudes (see e.g., Kuklinski, Cobb and Gilens, 1997; Kuklinski, et al., 1997; Holbrook and Krosnick, 2009; Janus, 2010; Weber et al., 2014; Bursztyn, Egorov and Fiorin, 2017). In a world where the social norm of racial-neutrality is mainstream, reporting a racial bias entails a social cost that racially-biased respondents may avoid by insincerely reporting no bias. However, the election of Donald Trump, a candidate with racially-biased views, signaled that the social norm of racial-neutrality was less mainstream than previously assumed. Consequently, the expected social cost of expressing racist attitudes decreased, making them *ceteris paribus* more likely to be reported (Bursztyn, Egorov and Fiorin, 2017). By the same token, the first election of Barack Obama signaled an increase in the social desirability of racial-neutrality and hence lowered the probability to report racially-biased attitudes, whereas elections confirming the incumbent did not provide novel information about social norms and hence did not affect the report of racially-biased attitudes.

The mechanism relies on the assumption that the election of Donald Trump, and those of his predecessors, did not affect race-related attitudes, but rather the likelihood of reporting them. While this assumption cannot be directly tested in our setting, further analysis corroborates its validity. We show that the treatment effect does not reflect a gradual change in race-related attitudes occurring around the election. In particular, neither pre-electoral campaign effects nor post-electoral bandwagon effects fully account for our main finding.

The nexus between Donald Trump’s win and the likelihood to report sensitive attitudes has found early empirical evidence in lab experiments (Huang and Low, 2017; Bursztyn, Egorov, and Fiorin, 2017; Crandall, Miller, and White 2018). We

contribute to that literature in two ways. Firstly, while Huang and Low (2017), Bursztyn, Egorov, and Fiorin (2017), and Crandall, Miller, and White (2018) rely on lab-experiments, we mimic a natural experiment design based on representative samples from observational data. Secondly, while those works focus on the impact of Donald Trump’s election on attitudes and norms of behavior in the U.S., we are the first to provide an exploratory analysis of the transnational contagion of racially biased attitudes.

2 Empirical analysis

2.1 Sample

The data we use come from Round 8 of the European Social Survey, which includes 18 countries. Donald Trump’s election fell inside the survey fieldwork period of 13 of them. The fieldwork periods, detailed in appendix, typically lasted three to four months. The survey is constructed using highly rigorous translation protocols and conditional monetary incentives are granted to units upon the completion of face-to-face interviews.

2.2 Empirical model

To identify racially biased attitudes, we use questions *B38* and *B39* of the *ESS*. Specifically, question *B38* reads, “*To what extent do you think the country should allow people of the same race or ethnic group as most people of the country to come and live here?*” Question *B39* directly follows question *B38* and reads, “*How about people of a different race or ethnic group from most people?*” Answers to both questions range from (1) “Allow many” to (4) “Allow none”.

Because the two questions only differ in the race dimension, the differences in answers can only be driven by differences in the perception of migrants according to their race. By giving different answers to the two questions, respondents therefore knowingly reveal a racial bias. Most of all, because interviews are conducted face-to-face, respondents are subject to a stronger social pressure than in internet surveys, where racially biased opinions are revealed anonymously (Seth, 2013).

Denoting respondent i 's opposition to different and same race immigration by y_{1i} and y_{2i} , respectively, the dependent variable “self-reported racial bias in policy attitudes”, y_i , is defined as a dummy variable taking the value 1 if $y_{1i} > y_{2i}$ and 0 otherwise. In the relevant sample, 32.70% of individuals report stronger opposition to different race immigration than same-race immigration, while 64.80% report equal opposition to different-race immigrants. While such operationalization of the dependent variable has the advantage of simplicity, we discuss its limits and test the robustness of our results to using two alternatives in appendix (B). Reports of racist attitudes ($y_i = 1$) account for 31.49% in the control group and 34.06% in the treatment group.¹

Defining $Y_{i,c} = \ln \left[\frac{\Pr(y_{i,c}=1)}{1-\Pr(y_{i,c}=1)} \right]$, we use the following specification:

$$Y_{i,c} = \alpha + \beta T_i + \gamma' X_{i,c} + \mu_c + \epsilon_{i,c}.$$

$T_i \in \{0, 1\}$ is the treatment variable. It takes the value 1 if respondent i was interviewed after November 8, 2016 and 0 otherwise. Even though asymmetric levels of respondents' reachability as well as geographic imbalance may induce a non-random selection of respondents among the control and treatment groups (Legewie, 2013; Munoz et al., 2018), the timing of each interview is as good as random with respect to the timing of the U.S. election. T_i can therefore be interpreted as an exogenous signal of the decrease in the social desirability of reporting racially-neutral attitudes. Accordingly, β measures the effect of Donald Trump's win on the propensity to report racist attitudes. α is a constant.

$X_{i,c}$ summarizes individual-level characteristics. In a first model, we only control for *demographic* characteristics including age, age squared, sex, household status (having at least one child living at home), and ethnic minority status (0 if majority, 1 if minority). We then add *socioeconomic* characteristics: highest education attainment (1-7), a dummy capturing economic insecurity (0 if the respondent experienced short run unemployment during the previous year, and 1 otherwise)

¹2.49% of respondents oppose same race immigration more than different race immigration, displaying “positive racism”. In an alternative specification, we allow the dependent variable to take the value -1 in the latter case. As the number of units reporting positive racism is extremely limited, treatment effects are very close in the two cases.

and household’s income (1 meaning living comfortably through 4 meaning living with strong difficulties). We subsequently add a dummy equal to 1 if the respondent voted in the latest general election, to capture interest in politics. Note that we only select proper covariates, i.e. covariates that could not be affected by the treatment. To control for unobserved country heterogeneity, we include country fixed effects μ_c . $\epsilon_{i,c}$ is an idiosyncratic error term, with $E[\epsilon | T, X, \mu] = 0$. Finally, we weight observations by the design weights provided by the *ESS* to control for the relative likelihood of each observation to be sampled.

Respondents in the treatment and control groups may differ in the distribution of key covariates. While the *ESS* is meant to be representative of each country’s population in the overall period, there is no guarantee that representativeness holds within particular sub-periods, for instance because of reachability issues. Following Hainmueller (2012), we therefore weight control units such that the distribution of covariates in the control group matches the moment conditions (until skewness) of the treatment group. After this pre-processing, covariate imbalance between control and treatment groups becomes negligible.

We fit the model with a binary logit estimator and report the average marginal effect. Since both the treatment and the output variables are dummies, the marginal effect is easy to interpret: It provides the difference in percentage points between the treated and control groups in the probability of a respondent exhibiting a racial bias. We base our main analysis on an interval of ± 15 days before and after the election. This bandwidth choice reflects a trade-off between statistical power, which is greater the larger the bandwidth, and attribution, which is more accurate the smaller the bandwidth. We discuss in greater details this rather arbitrary choice and report treatment effects for alternative bandwidths in appendix.

The ideal dataset to study a “global contagion” should include race-related attitudes of each individual in each country both before and after the election. Instead, we had to run the analysis on a sample of 13 countries. We therefore face sample uncertainty (Abadie et al., 2017). Moreover, race-related reports are observed either before or after the election. Consequently, we also face design uncertainty (Abadie et al., 2017). For these reasons, we cluster errors at the country level.

3 Results

3.1 Main test

Table 1 shows that the hypothesis that Donald Trump’s win increased global self-reported racial bias in policy attitudes cannot be rejected. In column (i), the treatment effect, computed as a simple mean-difference, is equal to 3.2 percentage points and statistically significant at $p < .01$. In column (ii), we control for country fixed effects only. Being interviewed after Donald Trump’s election increases the likelihood of reporting a racial bias by 1.8 percentage points, and the outcome is now significant at $p < .05$. Columns (iii) and (iv) add control variables pertaining, respectively, to demographic and socioeconomic characteristics. The treatment effect slightly exceeds 2 percentage points.

In column (v), we include self-reported turnout in the latest national election. The treatment effect is nearly unaffected and stays significant at $p < .01$. A key comparison is the one between columns (v) and (vi). When the control units are weighted to match the covariates’ distribution of treated units, the treatment effect hardly increases. This suggests that sample imbalance is not severe. In the full specification of column (vi), on which we are going to rely to address further identification issues, Donald Trump’s election increases the probability of reporting a racial bias by 2.3 percentage points, significant at $p < .01$.

The appendix digs deeper into the temporal and spatial dimensions of the treatment effect. As the lengths of the fieldwork periods are limited and different among countries, we remain agnostic about medium-run effects. However, we show that the main result is robust to alternative bandwidths. We also compare each country’s specific treatment effect with the aggregate one. We document that the treatment effect is significantly lower than average in Sweden, Finland and Estonia, suggesting the presence of a Scandinavian cluster of non-updaters. The treatment effect is significantly higher than average in Austria, Switzerland, and the Netherlands, suggesting the presence of a continental cluster of stronger updaters, paralleled by Israel.

We also perform several robustness checks. Firstly, we show that alternative modeling strategies as well as alternative operationalizations of the dependent

	Racial bias (0-1)					
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Treatment (0-1)	.031***	.017**	.020***	.021***	.022***	.023***
SE	(.011)	(.009)	(.008)	(.009)	(.009)	(.008)
N.obs	7,904	7,904	7,855	7,720	7,685	7,685
Country Effects		yes	yes	yes	yes	yes
Demographics			yes	yes	yes	yes
Socioeconomics				yes	yes	yes
Voting					yes	yes
Entropy balancing						yes

Coefficients for treatment effect: average marginal effects following Logit estimation. significant at .1, **: significant at .05, ***: significant at .01. Standard errors clustered at country level in each model. The analysis is based on 4,064 effective control and 3,653 effective treated units. Countries: Austria, Belgium, Switzerland, Germany, Estonia, Finland, UK, Israel, Norway, Sweden, and Slovenia. Demographics: age (15-105), age squared, gender (0-1), household status (0-1), minority status (0-1), and domicile (1-4). Socioeconomics: education attainment (1-7), income status (1-4), and a dummy capturing whether the respondent experienced short-run unemployment during the last year (0-1). Voting takes value one if the respondent voted at latest general election (0-1) Entropy balancing weights units in order for the distribution of covariates of the control group to match the distribution of covariates of the treated group, until skewness. Design weights apply. Source: ESS, round 8.

Table 1: Effect of Donald Trump’s election on self-reported racial bias.

variable yield qualitatively and quantitatively close treatment effects. Secondly, to deal with the sample selection issues that our pre-treatment matching strategy may not capture, we run the same analysis again after balancing for reachability and geographic imbalance. We obtain very similar treatment effects. These tests are reported in appendix.

3.2 Threats to identification

Trump v. previous elections. Our strategy rests on the contention that Donald Trump’s election marked a change in the *status quo* toward lower social desirability of racial equality. To see what would have happened to race-related attitudes if the *status quo* had changed toward higher racial equality, we apply our design to Barack Obama’s first election, which is arguably the closest to a perfect opposite of Donald Trump’s. Figure 1a shows that the effect of his first election was the opposite of Donald Trump’s: The report of racial bias in policy attitudes

decreased significantly at $p < .05$.²

A second question regarding counterfactuals is the following: What would have happened if the *status quo* had not changed? George W. Bush’s 2004 and Barack Obama’s 2012 elections represent appealing counterfactuals, since the incumbents were confirmed, hence there was no change in the *status quo*. Figure 1a shows that the effect of the 2004 and 2012 elections, which granted second mandates respectively to George W. Bush and Barack Obama, is not statistically distinguishable from zero.

Racist v. immigration attitudes. The two questions we combine to construct the dependent variable only differ in the racial background of immigrants. However, race-targeting policies may face opposition due to welfare concerns, rather than racism (Bobo and Kluegel, 1993). Respondents in the ESS may have simply used race as a proxy for specific labor market skills or the demand for public goods (Dustmann and Preston, 2007). In that case, expressing greater opposition to different v. same-race immigration would be driven by welfare concerns, rather than racism. However, the results shown in Figure 1b show that the documented effect on self-reported racial bias is not driven by economic-related immigration concerns. In appendix, we moreover show that policy attitudes less ostensibly related with Donald Trump’s campaign - including redistribution, environmental protection and gay rights - are left unaffected by the election.

Electoral v. campaign effect. Schaffner (2017) shows that being exposed to Donald Trump’s campaign increased individuals’ willingness to express xenophobic opinions against minorities. Morrison et al. (2018) moreover show that assault frequency increased on days and in cities where candidate Donald Trump campaigned. As the campaign was covered worldwide, his xenophobic rhetoric may

²One may argue that the informational content of Donald Trump’s win was stronger than the one of Barack Obama’s 2008 win. In the first case, the electoral outcome was unexpected given pre-electoral polls. In the latter case, the outcome was less unexpected: Barack Obama and John McCain had close approvals until October, but Barack Obama gained an edge over John McCain during the last month. This may make the election itself less informative. It cannot be denied, however, that the election of the first African-American President in the U.S. marked an important discontinuity in world politics from the perspective of a global audience.

also have changed the willingness to report racist attitudes abroad prior to the election. This would bias our treatment effect downward. However, Figure 1c establishes that moving the treatment one week, 15 days, three weeks, or 30 days before the actual election, keeping a symmetric interval of time around it, yields no significant treatment effect. Although Donald Trump’s rise in popularity during the campaign may have affected race-related attitudes, it does not threaten the validity of our estimates of the effect of his election *per se*.

Electoral v. bandwagon effect. The election of Donald Trump may have affected a broad set of political attitudes due to a standard bandwagon effect, leading individuals to rally with the winning opinion (Fleitas, 1971). The observed change in race-related attitudes may then simply reflect a wider alignment on the positions of Donald Trump or on the perceived new stance of the US. Figure 1b however shows that some of the most archetypal political attitudes, including left-right placement and the support for right-wing populist parties remained constant, suggesting no generalized bandwagon effect.

4 Conclusion

Our analysis combines a methodological and a substantive contribution. While the extant literature focuses on the effect of Donald Trump’s election on domestic social norms, we study its effect on social norms abroad and provide evidence consistent with a phenomenon of contagion. The study of norm diffusion in world politics is so far limited, as the field of international organization has focused on an institutional top-down channel, whereby local social norms in a country change following institutional decisions inspired or imposed by a focal country (Acharya, 2004; Klotz, 1995). Our paper suggests that another informational channel, based on the reaction of citizens to election results abroad, may also cause shifts in global norms. Which mechanisms underlie and moderate the contagion? How do the institutional and informational channels interact? These questions represent an interesting avenue for future research.

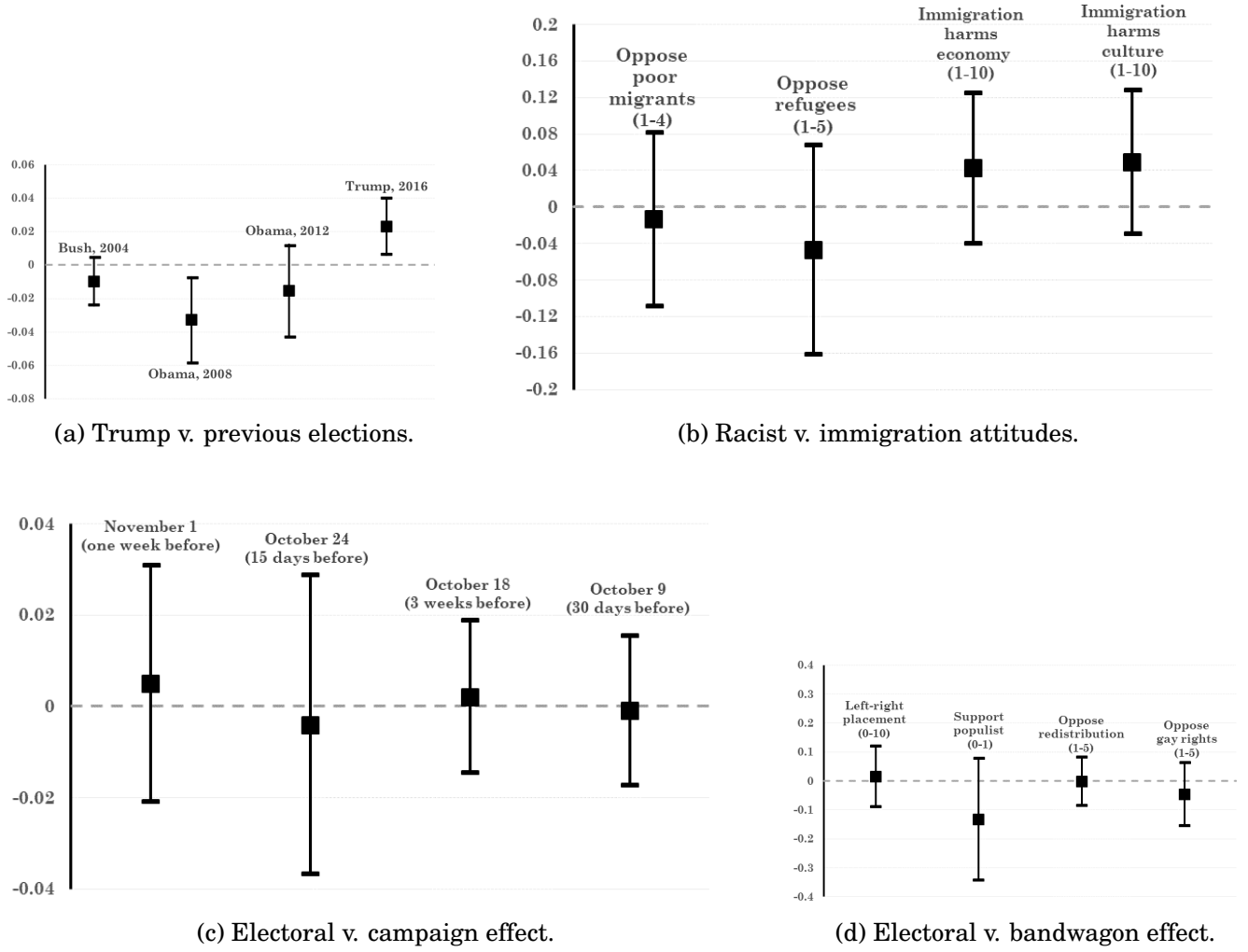


Figure 1: Threats to identifications: treatment effects with 95% confidence interval. Coefficients are computed according to model (vi) in Table 1. For discrete dependent variables, we report ordered Logit coefficients.

References

- [1] Abadie, Alberto, Athey, Susan, Imbens, Guido W. and Jeffrey Wooldridge. 2017. "Sampling-based vs. Design-based Uncertainty in Regression Analysis," (<https://economics.mit.edu/files/13161>).
- [2] Acharya, Amitav. 2004. "How Ideas Spread: Whose Norms Matter? Norm Localization and Institutional Change in Asian Regionalism," *International Organization* 58(2): 239-75.
- [3] Bar-Tal, D., and Labin, D. 2001. "The Effect of a Major Event on Stereotyping: Terrorist Attacks in Israel and Israeli Adolescents' Perceptions of Palestinians, Jordanians and Arabs." *European Journal of Social Psychology* 31(3): 265-80.
- [4] Bobo, Lawrence and James R. Kluegel. 1993. "Opposition to Race-Targeting: Self-Interest, Stratification Ideology, or Racial Attitudes?" *American Sociological Review* 58(4): 443-64.
- [5] Bursztyn, Leonardo, Georgy Egorov, and Stefano Fiorin, 2017. "From Extreme to Mainstream: How Social Norms Unravel," NBER working paper No. w23415.
- [6] Cameron Colin A., Gelbach, Jonah B. and Douglas L. Miller. 2008. "Bootstrap-based Improvements for Inference with Clustered Errors." *Review of Economics and Statistics* 90(3): 414-27.
- [7] Cherkaoui, Mohammed. 2016. "Donald Trump's Presidency: New Dawn or Dooms Day?" Al-Jazeera reports (<http://studies.aljazeera.net/en/reports/2016/11/donald-trumps-presidency-dawn-dooms-day-161127110643744.html>).
- [8] Crandall, Christian S., Miller, Jason M. and Mark H. White, 2018. "Changing Norms Following the 2016 US Presidential Election: The Trump Effect on Prejudice." *Social Psychological and Personality Science* 9(2): 186-92.

- [9] Dustmann, Christian and Ian P. Preston, 2007. "Racial and Economic Factors in Attitudes to Immigration," *B.E. Journal of Economic Analysis & Policy* 7(1).
- [10] Fleitas, Daniel W. 1971. "Bandwagon and Underdog Effects in Minimal Information Elections," *American Political Science Review* 65(2): 434-38.
- [11] Hainmueller, Jens, 2012. "Entropy Balancing for Causal Effects: A Multivariate Reweighting Method to Produce Balanced Samples in Observational Studies," *Political Analysis* (2012) 20: 25-46.
- [12] Hainmueller, Jens and Yiqing Xu, 2013. "ebalance: A Stata Package for Entropy Balancing," *Journal of Statistical Software* 54(7): 1-19.
- [13] Hauslohner, Abigail. 2016. "Hate crimes rose the day after Trump was elected, FBI data show." The Washington Post (https://www.washingtonpost.com/news/post-nation/wp/2018/03/23/hate-crimes-rose-the-day-after-trump-was-elected-fbi-data-show/?noredirect=on&utm_term=.9f63f017b548).
- [14] Holbrook, Allyson L., and Jon A. Krosnick. 2009. "Social desirability bias in voter turnout reports: Tests using the item count technique." *Public Opinion Quarterly* 74(1): 37-67.
- [15] Huang, Jennie and Corinne Low, 2017. "Trumping Norms: Lab Evidence on Aggressive Communication Before and After the 2016 US Presidential Election," *American Economic Review* 107(5): 120-24.
- [16] Iacus, Stefano, Gary King and Giuseppe Porro. 2012. "Causal Inference Without Balance Checking: Coarsened Exact Matching," *Political Analysis* 20: 1-24.
- [17] Klotz, Audie. 1995. "Norms Reconstituting Interests: Global Racial Equality and the U.S. Sanctions Against South Africa." *International Organization* 49 (3): 451-78.
- [18] Kuklinski, James H., Michael D. Cobb, and Martin Gilens. 1997. "Racial attitudes and the 'New South'," *The Journal of Politics* 59(2): 323-49.

- [19] Kuklinski, James H., Sniderman, Paul M., Knight, Kathleen, Piazza, Thomas, Tetlock, Philip E., Lawrence, Gordon R. and Barbara Mellers, 1997. "Racial Prejudice and Attitudes Toward Affirmative Action," *American Journal of Political Science* 41(2): 402-19.
- [20] Janus, Alexander L., 2010. "The Influence of Social Desirability Pressures on Expressed Immigration Attitudes," *Social Science Quarterly* 91(4): 928-46.
- [21] Legewie, Joscha. 2013. "Terrorist Events and Attitudes toward Immigrants: A Natural Experiment." *American Journal of Sociology* 118(5): 1199-245.
- [22] Levin, Brian H., and Kevin Grisham, 2017. "Hate crimes rise in major American localities in 2016," United States Department of Justice Hate Crime Summit, Washington, DC, June 29.
- [23] Mackinnon, James G. and Matthew D. Webb. 2017. "Wild Bootstrap Inference for Wildly Different Cluster Sizes," *Journal of Applied Econometrics* 32(2): 233-54.
- [24] Mikulashek, Christoph, Pant, Saurabh and Tesfaye, 2016. "Winning Hearts and Minds in Civil Wars: Governance, Leadership Change, and Support for Violence in Iraq," (http://christophmikulasc hek.com/wp-content/uploads/2016/12/Mikulasc hek-Pant-Tesfaye_2016.pdf).
- [25] Morrison, Christopher, Ukert, Benjamin, Palumbo, Aimee, Dong, Beidi, Jacoby, Sara, and Wiebe, Douglas, 2018. "Assaults on Days of Campaign Rallies During the 2016 US Presidential Election," *Epidemiology*, forthcoming.
- [26] Muñoz, Jordi, Falcó-Gimeno, Albert and Hernández, Enrique. 2018. "Unexpected Event During Surveys Design: Promise and Pitfalls." Available at SSRN: <https://ssrn.com/abstract=3159194> or <http://dx.doi.org/10.2139/ssrn.3159194>.
- [27] Müller, Karsten and Schwarz, Carlo. 2018. "Making America Hate Again? Twitter and Hate Crime Under Trump." Available at SSRN: <https://ssrn.com/abstract=3149103>.

- [28] Perrin, Andrew J., and Smolek, Sondra J. 2009. "Who Trusts? Race, Gender, and the September 11 Rally Effect among Young Adults." *Social Science Research* 38(1): 136-47.
- [29] Rushin, Stephen and Edwards, Griffin Sims. 2018. "The Effect of President Trump's Election on Hate Crimes." Available at SSRN: <https://ssrn.com/abstract=3102652>.
- [30] Shabi, Rachel, 2016. "How Europe's far-right feasts on Trump's victory," *Al Jazeera*, 17 November 2016 (<http://www.aljazeera.com/indepth/opinion/2016/11/europe-feasts-trump-victory-161116122158503.html>).
- [31] Schaffner, Brian F., 2017. "Follow the Racist? The Consequences of Expressions of Elite Prejudice for Mass Rhetoric," mimeo (<https://umass.app.box.com/s/x5zz210nor2z0v93m8frdlzxyobggrlm>).
- [32] Seth, Stephens-Davidowitz. 2013. "The Cost of Racial Animus on a Black Candidate: Evidence using Google Search Data," *Journal of Public Economics* 118(3): 26-40.
- [33] Weber, Christopher R., Lavine, Howard, Huddy, Leonie and Christopher M. Federico, 2014. "Placing Racial Stereotypes in Context: Social Desirability and the Politics of Racial Hostility," *American Journal of Political Science* 58(1): 63-78.

Appendix

A Descriptive statistics

- Table 3 provides the complete descriptive statistics for Donald Trump’s election. Table 4 provides descriptive statistics for previous elections.

B Robustness Checks

- **Imbalance: Mean and Variance.** Table 2 provides the descriptive statistics for each covariate used in the main analysis. It provides information about the imbalance in covariates between treated and control units before and after applying entropy balancing. Imbalance is somewhat limited in the aggregate analysis, as one can already suspect by looking in Table 1 at the difference between the treatment effect with and without entropy balancing weighting. The latter became extremely small once entropy balancing was applied.
- **Imbalance: Skewness.** Table 2 allows observing means and variances, but it does not provide information about skewness, which is only relevant for continuous covariates. Figure 2 provides the kernel density for the four continuous covariates used in the analysis. Some degree of skewness persists after balancing on the “age” variable, whereas it essentially disappears for domicile, education and income. For all of these covariates, the kernel density of the treated and control groups are almost indistinguishable.
- **Alternative specifications.** Table 5 provides additional specifications. As suggested by Hainmueller and Xu (2013), entropy balancing is preceded by the extraction of outliers operationalized through pre-treatment coarsened exact matching. We first run an imbalance test on covariates. We then match control and treated units with coarsened exact matching on imbalanced covariates within each country. Units without match are treated as outliers and

pruned before running the analysis. Details are provided in the table’s footnote. In column (i), we provide the treatment effect after outliers’ extraction and prior to entropy balancing. In column (ii), we apply entropy balancing after having extracted outliers. In both cases, the treatment effect is slightly larger than in column (vi) of Table 1. Column (iii) displays the outcome of an alternative, more ambitious, balancing strategy, in which entropy weighting is constructed at the country level. The treatment effect gets slightly larger under this specification.

- With this by country balancing strategy, however, we are not always able to achieve balance at the third moment. Column (iv) and (v) replicate the analysis of, respectively, columns (ii) and (iii), but account for the fact that, since the number of clusters is relatively low (13 countries), standard clustering may underestimate standard errors. We re-estimate the main result using a wild cluster bootstrap. The level of significance only decreases from $p < .01$ to $p < .05$ for the country-level balancing specification.
- Finally, in column (vi), we provide the treatment effect for an augmented set of covariates. In the main text, we deliberately restrict the set of controls to proper covariates. The effect of the treatment might therefore be driven by a shift in ideology. To rule that possibility out, we control for three further attitudes: left-right placement, political interest, and satisfaction with democracy. The treatment effect after outliers’ extraction, entropy balancing, and wild cluster bootstrapping is about 2 percentage points, significant at $p < .01$.
- **Alternative dependent variable.** Call y_{same} the answer to the “same race migrants” question and y_{diff} the answer to the “different race migrants” question. Recalling that higher values on the 1-4 scale mean higher opposition, we construct three different dependent variables.

1. *Main* (in the text):

$$y_i = \begin{cases} 1 & \text{if } y_{\text{diff}} > y_{\text{same}} \\ 0 & \text{else} \end{cases}$$

This approach has the advantage of simplicity, because reported treatment effects can be interpreted directly. It has the disadvantage of not capturing increases in racial bias for already racially biased individuals thereby underestimating its actual increase.

2. *Alternative I:*

$$y_i = y_{\text{diff}} - y_{\text{same}}$$

This alternative has the advantage of capturing the intensity of the racial bias. However, an already racially biased individual (say $y_{\text{diff}} = 3$, $y_{\text{same}} = 1$) increasing his racial bias ($y_{\text{diff}} = 4$) would count the same as a racially unbiased becoming racially biased. An increase in racial bias may accordingly be driven by a polarization effect.

3. *Alternative II:*

$$y_i = \begin{cases} 1 & \text{if } y_{\text{diff}} > y_{\text{same}} \\ 0 & \text{if } y_{\text{diff}} = y_{\text{same}} \\ -1 & \text{else} \end{cases}$$

In this case, we account for the fact that some respondents may favor different race over same race immigrants. This group is extremely limited (2.78% of our sample), which is why we used a dummy rather than a categorical variable in our baseline specification.

We estimated the baseline specification with each alternative definition of the dependent variable in Table 6. The three approaches lead to very close empirical conclusions.

- **Sampling selection issues: Reachability.** Pre-treatment matching accounts for imbalance in covariates among respondents. An additional issue arises as sampled individuals may be more or less likely to complete the survey before or after the election. The election of an American president *per se* unlikely induces self-selection. However, the respondents who completed the survey at the first attempt made by the interviewer are more likely to be in the control group than those for whom several attempts had to be made prior to completing the survey (Muñoz, Falcó-Gimeno and Hernández, 2018). This

is the issue of *reachability*: the more reachable a household, the more likely it is, *ceteris paribus*, to be interviewed before the election. Reachability would bias our results if it correlated with characteristics that we do not account for. The left panel of Figure 3 plots the fraction of households interviewed at the 1st, 2nd, n th attempt in the relevant control and treatment groups. The right panel considers the whole sample. Figure 3 confirms that the fraction of “easily reachable” respondents is indeed slightly higher in the control group. The average number of attempts to get the survey administered is 2.586 in the control and 2.659 in the treatment group.

To check whether and to which extent reachability biases outcomes, we run again the main regression controlling for the “reachability” variable, given by the number of attempts needed for the interviewer to interview the sampled household. We do this in two ways: (i) by controlling for reachability in the main regression, and (ii) by dropping individuals with particularly low levels of reachability. Figure 3 shows that treatment effects according to (i) and (ii) yield outcomes that are extremely close to those presented in the main table.

- **Sampling selection issues: Geographic imbalance.** For most countries, *ESS* implements strict probability sampling. Multi-stage is only used for countries lacking reliable addresses of households (see ESS8 Sampling guidelines, page 6). As a result, there is a decent balance of before/after collection by region: for most regions there are data both before and after the election (Figure 4). As some imbalance persists, and regions may correlate with features that our matching strategy does not capture, we run two additional analyses. In the first, we add region fixed effects. In the second, we also use entropy weighting to make the control and treatment groups balanced on the fraction of surveys collected in each region. Both those treatment effects are extremely close to baseline estimates (see Figure 5).

	Range	Treated		Control				Imbalance	
		(Unconditional)		(Unconditional)		(After balancing)		(Unconditional)	(After balancing)
		Mean	Variance	Mean	Variance	Mean	Variance	Δ Mean	Δ Mean
Age	15-120	48.91	313.9	49.48	337.8	48.91	313.9	-.57	0
Female	0-1	.52	.25	.52	.25	.52	.25	0	0
Domicile	0-3	1.72	1.62	1.86	1.59	1.72	1.62	-.14	0
Household status	0-1	.67	.22	.68	.22	.67	.22	0	0
Minority status	0-1	.06	.05	.05	.05	.06	.05	.01	0
Education attainment	1-7	4.22	2.89	4.28	2.96	4.22	2.89	-.06	0
Income	1-4	1.87	.64	1.88	.66	1.87	.64	.01	0
Unemployment	0-1	.73	.19	.73	.19	.73	.19	0	0
Voted at latest election	0-1	.72	.20	.72	.20	.72	.20	0	0

Table 2: **Imbalance: 1.** Descriptive statistics and imbalance: before and after entropy balancing.

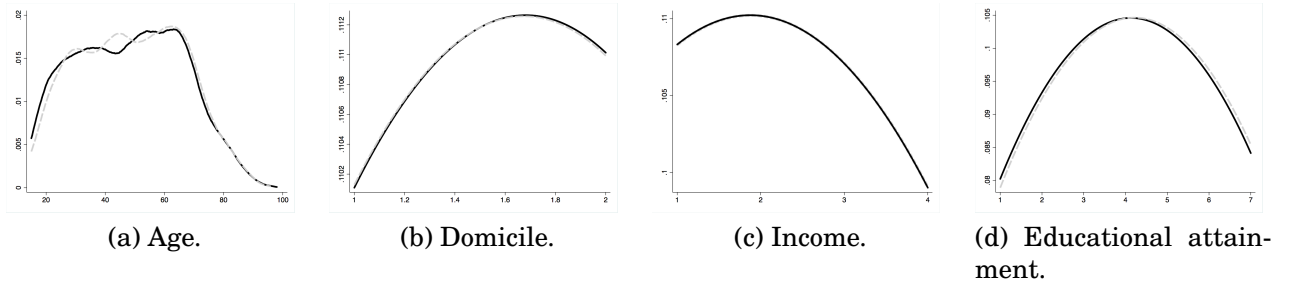


Figure 2: **Imbalance: 2.** Kernel density of continuous covariates among treatment and control groups after entropy balancing. The black regular line (grey dashed) plots treated (control) units.

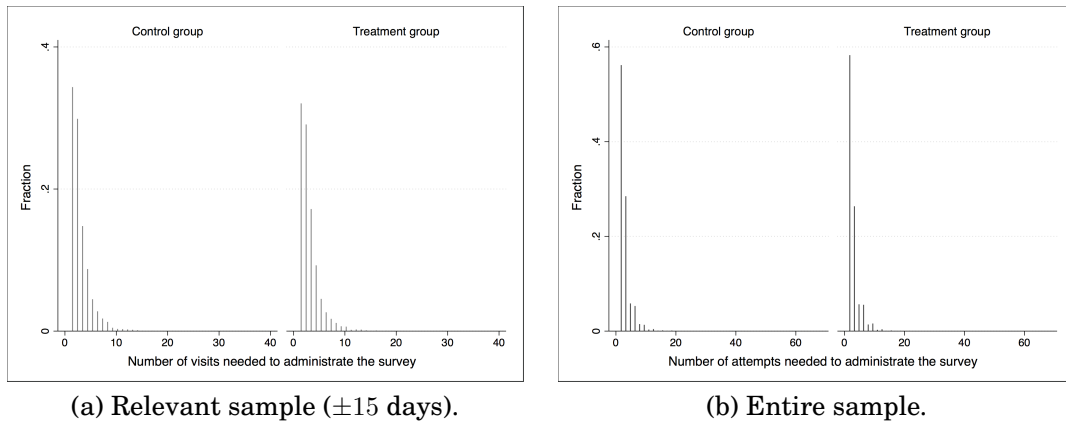


Figure 3: Reachability in the control and treatment group.

Dependent Variables	N. obs	Mean	Std. Dev	Min	Max
Racial bias	7,904	.33	.47	0	1
Same race immigration	7,951	2.16	.87	1	4
Different race immigration	7,929	2.57	.91	1	4
Oppose refugees	7,923	3.39	1.78	1	5
Oppose poor migrants	7,906	2.62	.92	1	4
Immigration harms economy	7,848	4.94	2.45	0	10
Immigration harms culture	7,895	5.17	2.64	0	10
Left-right placement	7,415	5.21	2.18	0	10
Support Populist	3,710	.16	.37	0	1
Oppose Redistribution	7,950	2.36	1.07	1	5
Oppose Gay rights	7,861	2.78	1.35	1	5
Independent Variables	N. obs	Mean	Std. Dev	Min	Max
Age	8,035	48.36	18.42	15	105
Female	8,053	.52	.50	0	1
Children at home	8,053	.32	.47	0	1
Minority status	8,053	.96	.23	0	1
Domicile	8,047	2.78	1.26	1	5
Income	7,981	1.88	.81	1	4
Education	8,013	4.08	1.72	1	7
Unemployed	8,033	.26	.44	0	1
Voting	8,023	.69	.46	0	1

Table 3: Descriptive statistics, Donald Trump (2016).

Dependent Variables	N. obs	Mean	Std. Dev	Min	Max
Racial bias (Bush, 2004)	11,269	.25	.43	0	1
Racial bias (Obama, 2008)	7,402	.27	.44	0	1
Racial bias (Obama, 2012)	8,208	.31	.46	0	1
Independent Variables (Bush, 2004)	N. obs	Mean	Std. Dev	Min	Max
Age	11,637	45.86	18.59	15	99
Female	11,673	.52	.50	0	1
Children at home	11,672	.41	.49	0	1
Minority status	11,676	.96	.20	0	1
Domicile	11,683	3.01	1.17	1	5
Income	11,533	1.97	.85	1	4
Education	11,661	3.32	1.72	1	5
Unemployed	11,633	.25	.43	0	1
Voting	11,600	.70	.46	0	1
Independent Variables (Obama, 2008)	N. obs	Mean	Std. Dev	Min	Max
Age	7,617	47.35	18.57	15	123
Female	7,629	.52	.50	0	1
Children at home	7,586	.37	.48	0	1
Minority status	7,605	.95	.21	0	1
Domicile	7,615	2.90	1.23	1	5
Income	7,617	1.84	.80	1	4
Education	7,563	3.13	1.37	1	5
Unemployed	7,586	.25	.43	0	1
Voting	7,589	.74	.44	0	1
Independent Variables (Obama, 2012)	N. obs	Mean	Std. Dev	Min	Max
Age	8,506	48.36	18.42	15	96
Female	8,519	.53	.50	0	1
Children at home	8,516	.39	.49	0	1
Minority status	8,461	.93	.26	0	1
Domicile	8,510	2.79	1.24	1	5
Income	8,466	1.73	.87	1	4
Education	8,399	4.11	1.81	1	7
Unemployed	8,449	.26	.44	0	1
Voting	8,464	.771	.45	0	1

Table 4: Descriptive statistics, other elections.

	Racial bias (0-1)					
	(i)	(ii)	(iii)	(iv)	(iv)	(v)
Treatment (0-1)	.024***	.025***	.025***	.025***	.025**	.020***
SE	(.008)	(.008)	(.011)	NA	NA	NA
N.obs	7,301	7,301	7,301	7,301	7,301	6,694
Country Effects	✓	✓	✓	✓	✓	✓
Demographics	✓	✓	✓	✓	✓	✓
Socioeconomics	✓	✓	✓	✓	✓	✓
Voting	✓	✓	✓	✓	✓	✓
Outliers' extraction (CEM)	✓	✓	✓	✓	✓	✓
Entropy balancing (pooled)		✓		✓		✓
Entropy balancing (by country)			✓		✓	
Wild Cluster bootstrapping				✓	✓	✓
Further political attitudes						✓

*: significant at .1, **: significant at .05, ***: significant at .01. Coefficients: average marginal effects following a logit estimation. Errors clustered at country level. Countries: Austria, Belgium, Switzerland, Czech Republic, Germany, Estonia, Finland, UK, Israel, the Netherlands, Norway, Sweden, and Slovenia. Demographics: age, age squared, gender, household status, minority status, and domicile. Socioeconomics: education attainment, income, and recent short-run unemployment. Entropy balancing designed to satisfy moment conditions until skewness. Outliers' extraction following coarsened exact matching on imbalanced covariates (Age and education). Age is coarsened through intervals of 5 years while domicile is coarsened according to Scott-break algorithm. Matching prunes 457 units (270 controls). Wild cluster bootstrapping is run through OLS after 1000 successful resamples. Further political attitudes include: left-right placement (0-10), political interest (1-4) and satisfaction with democracy (0-10). Design weights apply. Source: ESS, round 8.

Table 5: Alternative specifications. Effect of Donald Trump's election on self-reported racial bias, further specifications.

	<i>Alternative I</i>	<i>Alternative II</i>	MAIN
	(1-7)	(-1, 0, 1)	(0,1)
Treatment (0-1)	.123***	.119***	.122***
SE	(.043)	(.040)	(.044)
N. Obs	7,717	7,717	7,717
Country Effects	✓	✓	✓
Demographics	✓	✓	✓
Socioeconomics	✓	✓	✓
Voting	✓	✓	✓
Entropy balancing	✓	✓	✓

*: significant at .1, **: significant at .05, ***: significant at .01. Coefficients: Ordered Logit coefficient for Alternative I and Logit coefficient for Alternative II and MAIN. Errors clustered at country level. Countries: Austria, Belgium, Switzerland, Germany, Estonia, Finland, UK, Israel, Norway, Sweden, and Slovenia. Demographics: age, age squared, gender, household status, minority status, and domicile. Socioeconomics: education attainment, income, and recent short-run unemployment. Entropy balancing to satisfy moment conditions until skewness. Design weights apply. Source: ESS, round 8.

Table 6: Effect of Donald Trump's election on alternative dependent variables.

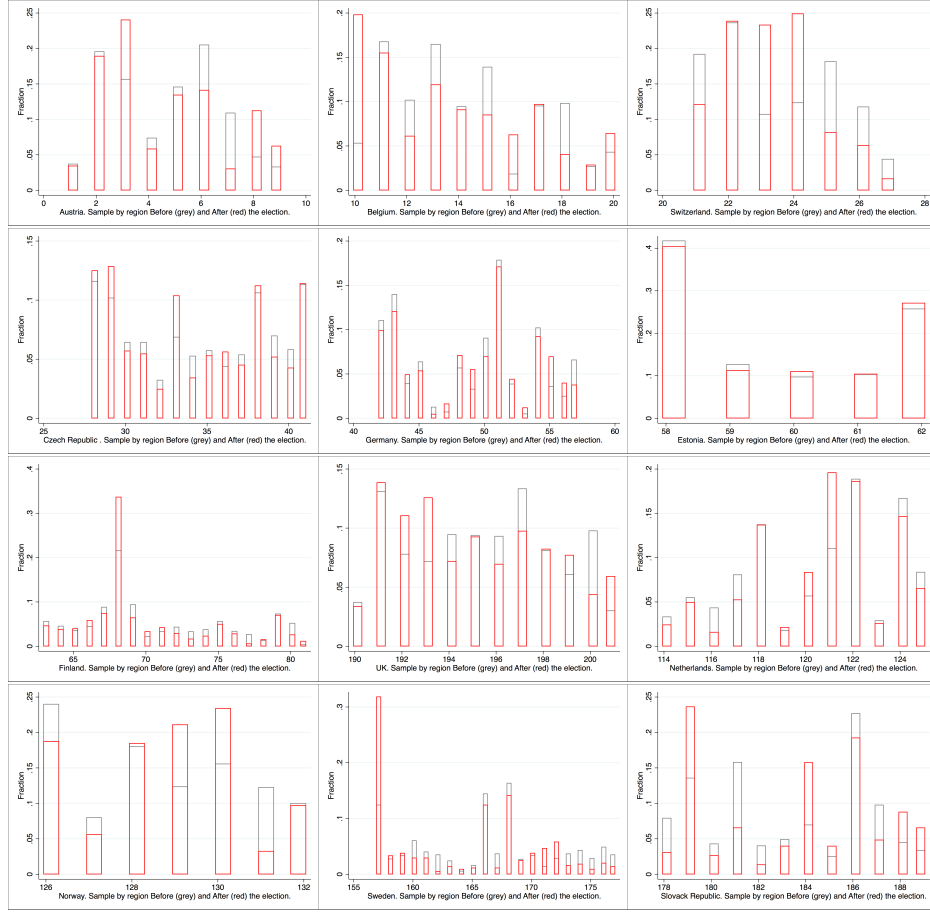


Figure 4: Survey collection by region before and after the election. No information available for Israel.

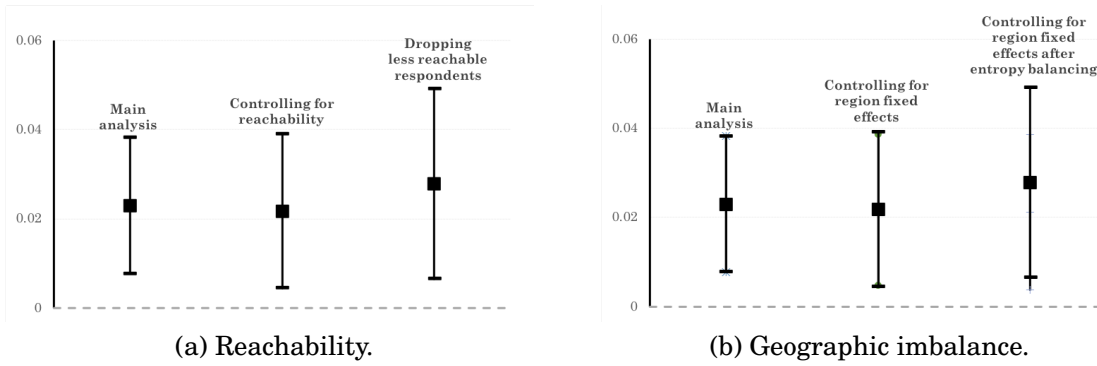


Figure 5: Treatment effects accounting for further sampling issues.

- **Trump v. previous elections.** Table 7 shows that, apart from Barack Obama’s 2008 election, none of the previous elections, studied in exactly the same way, caused any change in self-reported racial bias. The first three columns report the coefficient plotted in Figure 1a. In the last three columns, we show that the treatment effect of Donald Trump’s election is not only positive and significant, but also significantly larger than the ones from previous elections. We define $D_i \in \{0, 1\}$ as a dummy variable taking the value 1 if unit i was interviewed in 2016 and 0 if she was interviewed in a previous round (2004, 2008 or 2012). Following our main specification, we can write the empirical model as

$$Y_{i,c,p} = \alpha + \beta T_{i,p} + \delta_0 D_i + \delta_1 D_i \times T_{i,p} + \gamma' X_{i,c,p} + \mu_{c,p} + \epsilon_{i,c,p}.$$

where we control for country-year fixed effects, $\mu_{c,p}$, and cluster errors at the country-year level. We report the main coefficient of interest, δ_1 , which is a difference in difference estimate. As an alternative, we can also test whether the treatment effect following Donald Trump’s election according to the main specification is significantly larger than the one following previous elections with a z – test. Comparing the treatment effect following Donald Trump’s election with that of George W. Bush 2004, and those of Obama 2008 and 2012, we obtain respectively $z = -3.04$, $z = -3.56$, and $z = -2.44$. Hence the treatment effect of Donald Trump’s election is significantly larger than the one in previous elections in a one-sided test at $p < .01$.³

- **Electoral v. Campaign effect.** Table 8 reports the treatment effects that would be obtained applying the same method as in the baseline estimations around placebo election dates. We moved the treatment by intervals of 5 days until 30 days before the actual election day and kept symmetric intervals to

³As an additional test, we test whether this outcome holds true when restricting the sample to countries common to both elections. This exercise cuts substantially the sample, as the set of countries that happened to be fielded during U.S. elections differs from round to round. Nevertheless, the outcome remains significant. Comparing regression coefficients on common countries only yields $z = -2.11$, $z = -2.45$, and $z = -1.66$, when comparing the treatment effect following Donald Trump’s election with George W. Bush 2004, and Barack Obama 2008 and 2012. Hence, the comparison is significant at $p < .05$ in the first case, $p < .01$ in the second case, and $p < .1$ in the last case.

avoid the inclusion of actually treated units. None of the placebo treatment-dates before the real election yielded any change in self-reported racial bias.

- **Racist v. Immigration attitudes.** Table 9 reports estimates of the effect of Donald Trump’s election on four other survey items related to immigration. Those items read:

- Question C: The government should be generous in judging people’s applications for refugee status. (1: Agree strongly, ... , 5: Disagree strongly);
- Question B40: Allow many/few immigrants from poorer countries outside Europe. (1: Allow many, ... , 4: Allow none);
- Question B41: Would you say it is generally bad or good for [country]’s economy that people come to live here from other countries? (1: Bad, ... , 10: Good);
- Question B42: Would you say that [country]’s cultural life is generally undermined or enriched by people coming to live here from other countries? (1: Undermined, ... , 10: Enriched).

Consistent with our main result, we find no effect of Donald Trump’s election on welfare-related immigration attitudes, while we find that the only significant treatment effect is on cultural concerns for immigration (at $p < .1$)

- **Electoral v. Bandwagon effect.** Table 9 provides the outcome relative to the effect of Donald Trump’s election on four other survey items related to ideology. Those items read:

- Question B26: Placement on a left-right scale. (1: Left, ... , 10: Right);
- Question B24: Which party do you feel close to. (0: Any or none, ... , 1: a right-wing populist party);
- Question B23: Government should reduce differences in income levels. (1: Agree strongly, ... , 5: Disagree strongly);
- Question B36: Gay and lesbian couples should have the same rights to adopt children as straight couples. (1: Agree strongly, ... , 5: Disagree strongly).

We find that the treatment effect is null for each of these cases. We can therefore interpret our main finding as signaling that Donald Trump’s election had no general effect on the opinions of respondents. It therefore specifically increased the willingness to report opinions that discriminate migrants of a different race.

- **Unrelated issues.** Figure 6 shows that the election of Donald Trump had no effect on policy preferences for redistribution, LGBT rights, and environmental protection. The relevant proxies for those policy items are the following:

Issue I (Redistribution): Government should reduce differences in income levels. 1: Agree strongly; ... ; 5 Disagree strongly.

Issue II (Environmental protection): Important to care for nature and environment. 1: Very much like me; ... ; 6: Not at all like me.

Issue III (LGBT rights): Gays should live their life as they wish. 1: Agree strongly; ... ; 5 Disagree strongly.

- Following Legewie (2013), we randomly re-assign the binary treatment variable within countries. Internal validity is strengthened if the number of cases in which the random treatment effect exceeds the actual one is limited. This test helps ruling out a decisive role of sampling issues in driving the documented treatment effect. Testing for “permuted treatment effects” on the main dependent variable reveals that the number of cases in which the random treatment effect exceeds the actual one is only 3.2% after 1,000 Monte-carlo simulations.

C Further Analysis

- **Alternative bandwidths.** Following Depetris-Chauvín and Durante (2017), Giani (2017) and Mikulasche, Pant and Tesfaye (2017), we base our main analysis on an interval of ± 15 days before and after the election. On the one hand, the chosen interval balances out two necessities. *ESS* questionnaires feature a large number of questions, with no collection during weekends. The

rate of data collection per country is therefore relatively small. This requires selecting a sufficiently wide time interval. On the other hand, as race-related attitudes may vary according to several channels and further events, the observed treatment effects can be credibly attributed to the election outcome only if intervals are sufficiently close to the election day.

- To make sure that our results are not driven by the choice of the time interval, Table 10 provides the treatment effects over alternative intervals. The magnitude of the effect remains similar throughout the first month. If we consider an interval of ± 45 days, the treatment effect is still positive and significant at $p < .1$, and stays so exactly until Christmas day, while it becomes null when using larger time intervals, *e.g.* a time interval of ± 60 days.

We deliberately remain agnostic in what regards medium-term treatment effects for three reasons. Firstly, we can only credibly attribute observed changes to transnational electoral spillovers by focusing on the very short run, especially in light of the additional analysis presented in subsection 2.3. Conversely, longer intervals of time make confounding factor possibly decisive, and hence attribution problematic. Secondly, our objective is to disentangle an “electoral effect” from other effects that may concur to the evolution of race-related attitudes, including learning. Thirdly, as Figure 8 details, if we use an interval of ± 15 days, each country’s fieldwork period is active. Conversely, using an interval of *e.g.* ± 45 days, Austria and the Czech Republics’ fieldworks are closed. Hence, the increase in sample size from the ± 30 days interval to the ± 45 days interval is driven by a set of countries different from the one considered in the main analysis, thereby introducing a confounding factor.

For these reasons, the observed difference in the treatment effect over different study windows is consistent with different interpretations. It may be that the observed drop in the treatment effect when the bandwidth includes the Christmas holidays is due to Christmas itself. In line with this interpretation, we observe the same phenomenon in 2004 and 2012 (there are no data for 2008). In turn, this “Christmas effect” could be due both to the different

likelihood of different households to be willing to complete surveys during Christmas holidays, or to a mitigating effect of religious values on racial bias. It may also be that other events for which we are not accounting, possibly unrelated to American Politics, imposed further changes to race-related attitudes. Finally, it may also be due to the fact that contagion is a rather short-lived phenomenon, meaning that as the salience of the U.S. presidential election decreased, race-related attitudes returned to their original levels.

- **Analysis by country.** Figure 8 provides the timing of the interviews for each of the countries used in our analysis. The 2016 election of Donald Trump fell inside the survey period range of 14 of them. Iceland, however, has only 5 respondents before the election and was therefore discarded. Table 11 interacts the main dependent variable with each country dummy at a time. It shows that our main result is always significant at least at $p < .05$, and hence is not driven by outliers. Austria, Israel, the Netherlands, and Norway have a significantly higher treatment effect than the average, whereas in Estonia, Finland, Sweden, and Slovenia the outcome is significantly lower than the average. It would be interesting to run a comparative analysis, and identify the country-level variables that shape country-level treatment effects. However, the number of control and treated units per country is small when focusing on the relevant interval of time, making this analysis difficult.
- **Online search.** Figure 9 compares trends of Google searches in our sample of countries and in the U.S. for both “Trump” (Figure 9a) and “Racism” (Figure 9b). In both cases and for both geographic units, searches are the highest on November 9, the after-election day, confirming that Donald Trump’s election was connected with racism. Searches of Trump are more concentrated on November 9, 2016, for our sample than for the U.S., while the opposite is true for “racism” (and its translation in each country’s language).

	Racial bias (0-1)					
	Bush 2004	Obama 2008	Obama 2012	Trump-Bush	Trump-Obama	Trump-Obama
Treatment (0-1)	-.010	-.032**	-.017	.032***	.055***	.034**
SE	(.007)	(.013)	(.014)	(.011)	(.015)	(.015)
N.obs	10,817	7,191	7,894	18,108	14,908	15,661
Country Effects	✓	✓	✓	✓	✓	✓
Demographics	✓	✓	✓	✓	✓	✓
Socioeconomics	✓	✓	✓	✓	✓	✓
Voting	✓	✓	✓	✓	✓	✓
Entropy balancing	✓	✓	✓	✓	✓	✓

+: significant at .1, **: significant at .05, ***: significant at .01. Coefficients: average marginal effects following a logit estimation. Errors clustered at the country level and country-year for the difference in difference analysis. Demographics: age, age squared, gender, minority status, household status, and domicile. Socioeconomics: education attainment, income, and recent short-run unemployment. Education attainment ranges from 1 to 5 for Bush 2004 and Obama 2008. Entropy balancing is defined to satisfy moment conditions until skewness, separately for each round of the survey. Design weights apply. Bush 2004: Belgium, Switzerland, Czech Republic, Germany, Denmark, Estonia, Spain, Finland, UK, Luxembourg, the Netherlands, Norway, Poland, Portugal, Slovenia, Sweden, and Slovakia. Obama 2008: Switzerland, Cyprus, Germany, Denmark, Spain, Finland, France, UK, Israel, the Netherlands, Norway, Portugal, Slovenia, and Sweden. Obama 2012: Austria, Belgium, Switzerland, Cyprus, Germany, Estonia, Finland, UK, Ireland, Israel, Island, the Netherlands, Norway, Poland, Portugal, Russia, Sweden, Slovenia, and Slovakia. Source: ESS, rounds 2 - 4 - 6 - 8.

Table 7: Trump v. previous elections. Effect of *past elections* on self-reported racial bias.

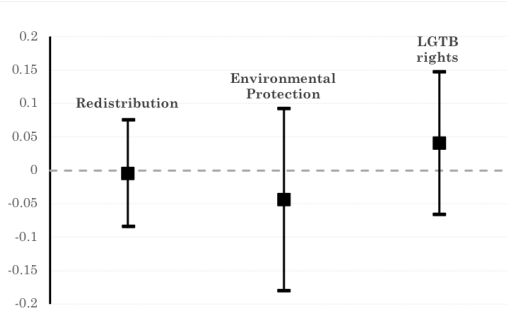


Figure 6: Effect of Donald Trump's election on unrelated "placebo" issues.

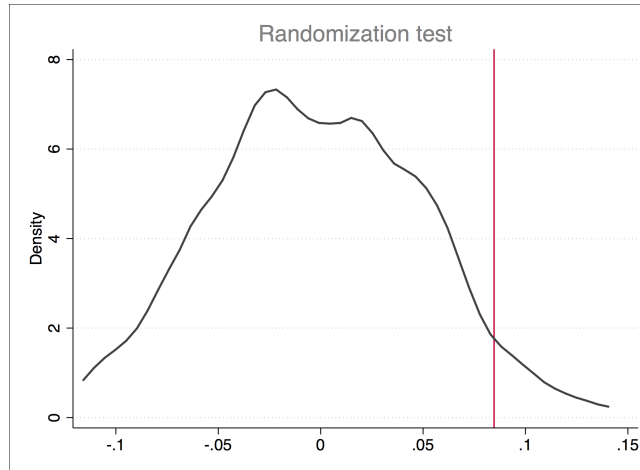


Figure 7: Kernel density of the permuted treatment effects.

Fake treatment date	Racial bias (0-1)			
	November 1	October 24	October 18	October 9
Treatment (0-1)	.008	-.008	-.000	-.005
SE	(.013)	(.015)	(.008)	(.009)
N. Obs	3,773	6,512	9,697	11,961
Country Effects	✓	✓	✓	✓
Demographics	✓	✓	✓	✓
Socioeconomic	✓	✓	✓	✓
Voting	✓	✓	✓	✓
Entropy balancing	✓	✓	✓	✓

*: significant at .1, **: significant at .05, ***: significant at .01. Coefficients: average marginal effects following Logit estimation. Countries: Austria, Belgium, Switzerland, Germany, Estonia, Finland, UK, Israel, Norway, Sweden, and Slovenia. Demographics: age, age squared, gender, household status, minority status, and domicile. Socioeconomics: education attainment, income, and recent short-run unemployment. Entropy balancing designed to satisfy moment conditions until skewness. Design weights apply. Source: ESS, round 8.

Table 8: **Electoral v. Campaign effect.** Effect of *fake treatments* election on self-reported racial bias.

	Racist v. Immigration attitudes			
	Oppose	Oppose	Immigration	Immigration
	refugees	poor migrants	harms economy	harms culture
	(1-4)	(1-4)	(0-10)	(0-10)
Treatment (0-1)	-.047	.002	.042	.049
SE	(.058)	(.053)	(.042)	(.040)
N. Obs	7,733	6,648	7,661	7,712
	Electoral v. Bandwagon effect			
	Left-right	Support	Oppose	Oppose
	placement	Populist	Redistribution	gay rights
	(1-10)	(0-1)	(1-5)	(1-5)
Treatment (0-1)	.014	-.009	-.003	-.047
SE	(.053)	(.016)	(.043)	(.055)
N. Obs	7,258	3,239	7,753	7,675
Country Effects	✓	✓	✓	✓
Demographics	✓	✓	✓	✓
Socioeconomic	✓	✓	✓	✓
Voting	✓	✓	✓	✓

*: significant at .1, **: significant at .05, ***: significant at .01. Coefficients: outputs from ordered logit regressions. Errors clustered at the country level. Countries: Austria, Belgium, Switzerland, Germany, Estonia, Finland, UK, Israel, Norway, Sweden, and Slovenia. Demographics: age, age squared, gender, household status, minority status, and domicile. Socioeconomics: education attainment, income, and recent short-run unemployment. Entropy balancing to satisfy moment conditions until skewness. Design weights apply. Source: ESS, round 8.

Table 9: **Racist v. Immigration attitudes and Electoral v. Bandwagon effect.** Effect of Donald Trump's Election on ideology and further immigration-related attitudes.

Racial bias (0-1)							
	7 days	15 days	21 days	30 days	45 days	60 days	All
Treatment (0-1)	.019**	.023***	.014***	.013*	.010*	.001	-.011
SE	(.009)	(.008)	(.006)	(.007)	(.006)	(.006)	(.010)
N. Obs	3,879	7,717	10,166	13,917	18,278	21,161	23,757
Country Effects	✓	✓	✓	✓	✓	✓	✓
Demographics	✓	✓	✓	✓	✓	✓	✓
Socioeconomic	✓	✓	✓	✓	✓	✓	✓
Voting	✓	✓	✓	✓	✓	✓	✓
Entropy balancing	✓	✓	✓	✓	✓	✓	✓

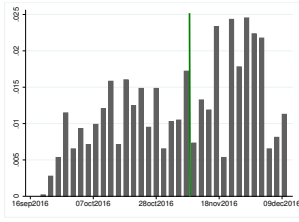
*: significant at .1, **: significant at .05, ***: significant at .01. Coefficients: average marginal effects following a logit estimation. Errors clustered at the country level. Countries: Austria, Belgium, Switzerland, Germany, Estonia, Finland, UK, Israel, Norway, Sweden, and Slovenia. Demographics: age, age squared, gender, household status, minority status, and domicile. Socioeconomics: education attainment, income, and recent short-run unemployment. Entropy balancing satisfies moment conditions until skewness. Design weights apply. Source: ESS, round 8.

Table 10: Effect of Donald Trump’s election on self-reported racial bias, by time interval.

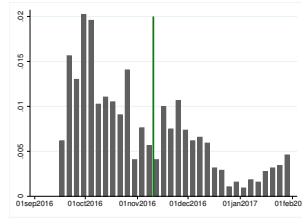
Racial bias (0-1)							
	AT	BE	CH	CZ	DE	EE	FI
Treatment	.020**	.023**	.023***	.021**	.023***	.027***	.028***
SE	(.008)	(.008)	(.008)	(.010)	(.009)	(.009)	(.007)
Treatment × Country	.021***	.005	-.004	.008	.002	-.033***	-.054***
SE	(.008)	(.009)	(.008)	(.011)	(.009)	(.008)	(.007)
	UK	IL	NL	NO	SE	SI	
Treatment	.023***	.020***	.023***	.020***	.024***	.024***	
SE	(.008)	(.008)	(.009)	(.008)	(.008)	(.008)	
Treatment × Country	-.002	.036***	.027***	.094***	xenophobic	-.006	
SE	(.008)	(.009)	(.009)	(.008)	(.007)	(.008)	

*: significant at .1, **: significant at .05, ***: significant at .01. Coefficients: average marginal effects following a logit estimation. Errors clustered at the country level. Entropy balancing to satisfy moment conditions until skewness. Design weights apply. Source: ESS, round 8.

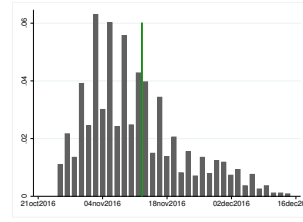
Table 11: Effect of Donald Trump’s election on self-reported racial bias, interacting with each country.



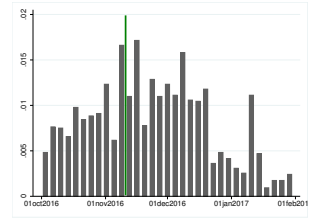
(a) Austria



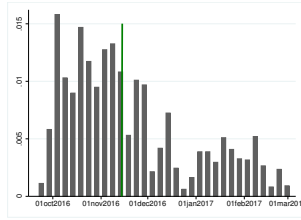
(b) Belgium



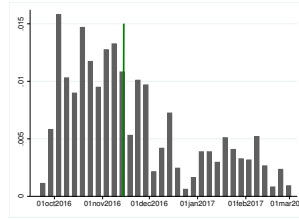
(c) Czech Republic



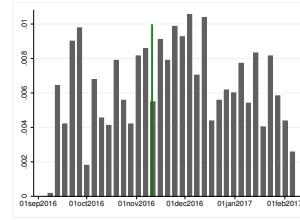
(d) Estonia



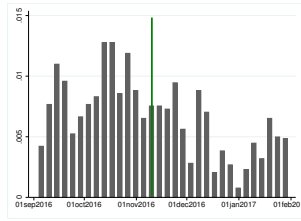
(e) Finland



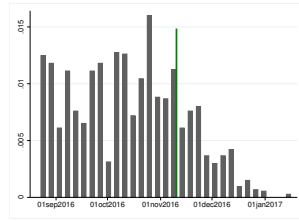
(f) Germany



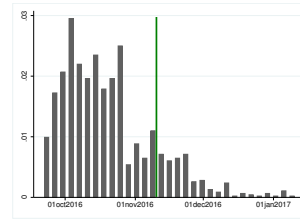
(g) Israel



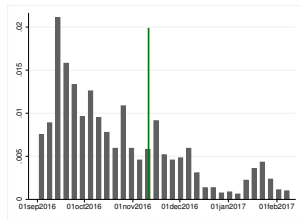
(h) Netherlands



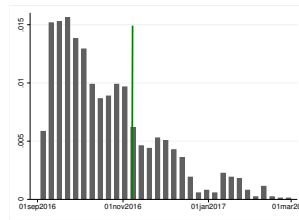
(i) Norway



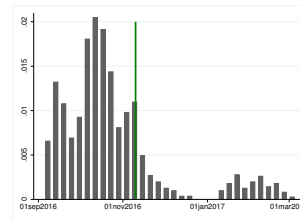
(j) Slovenia



(k) Sweden

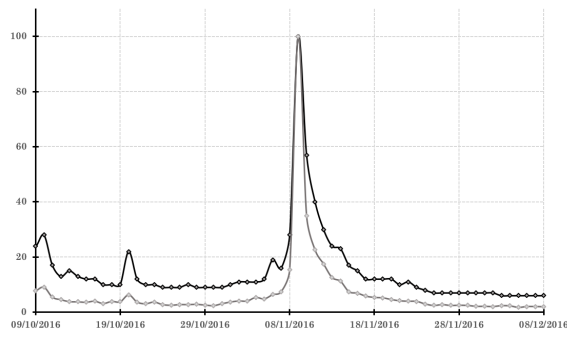


(l) Switzerland

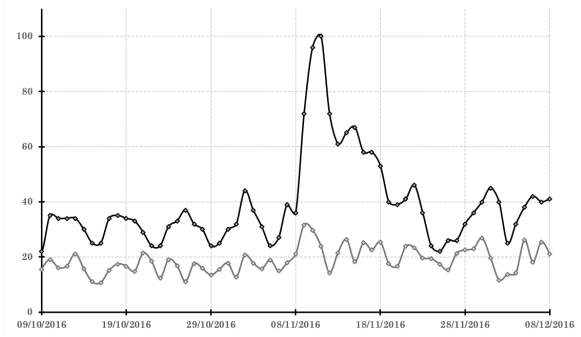


(m) UK

Figure 8: Data distribution by country. The green line represents the date of the US Presidential election (November 8, 2016).



(a) Trump.



(b) Racism.

Figure 9: Google trends on “Trump” and “racism” one months before and after the election. Black line: U.S, grey line: our sample. Units in the y -axis use information on search traffic on Google browser to compute means relative to an arbitrary initial value with respect to which each data point is scaled. For our sample, we first collect data for each country and then average them out. For “racism”, we also collect the country translation (*e.g.* for Germany, we separately collected “racism” and “rassismus”, and averaged them out).